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ABSTRACT

Written in response to a paper by Wayne K. Hoy on theory and practice in educational administration, this paper maintains that research and practice must be coordinated if educational administration is to progress. The author outlines Hoy's key points, agreeing with many but taking issue with Hoy's central thesis, which the author perceives as being that any effort to do research in an attempt to solve a practical problem is unscientific. The author disagrees particularly with the notion that scientific research in educational administration ought not to have practical application. He contends that neither an empirical nor a phenomenological approach is sufficient but that it is the explanatory model based on both the operational and representational models that is at the heart of theory development. (Author/JM)

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The Practice-Research Relationship in Educational Administration: Potion or Poison?

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The controversy between research and practice in education has raged for as long as anyone seems to remember. Nowhere has it been more vehement or divisive than in educational administration. Those who are concerned with practice often have no time or interest in even reading research (much less doing it) because of what they term the "ivory-tower," impractical, or sometimes even detrimental conclusions of the research. Those who are researchers in educational administration often think of themselves as somehow better (or at least brighter) than practitioner-oriented persons, who are assumed to be concerned with the "nuts and bolts" aspects of administration. Thus, we have continued in educational administration, fanning the flames of mistrust and dislike between researchers and practitioners. It seems to me that, although likely unintended, Hoy's article (1978) will provide another bellows to fan that flame.

Given the way Hoy presents his point of view regarding research and practice, it is difficult to contest any part of it without appearing to side with the anti-intellectual, atheoretical, and unscientific. In spite of the risk, I intend to take exception to Joy's position, that if research suggests some solution to a practical problem, it is not scientific and at best, second class research. Hoy doesn't come right out and say that; in fact, he says in his conclusion that he is not arguing against practical research. But his overall message says, to me at least, that any effort to do research in an attempt to solve a practical problem is unscientific, I disagree. Rather, I would take the position stated by Riffel (1978, p. 140) that one important". . . use of theory is executive, providing a basis of thoughtful, deliberated and directed action. Thus, a primary justification for the study of educational administration is a concern for the improvement of practice." But, let me begin by noting the many places where I agree with Hoy's position and thereby take some of the sting out of the possible charge that I am, if not in fact an anti-intellectualist, at least a fellow traveler.

I agree that science, research, and the university are best suited to problem-generation as opposed to problem-solving. A major fault in educational research is the fact that we often tend to test weak hypotheses with powerful statistics. That is, we begin by posing the wrong hypotheses, or asking the wrong question. This is even more dangerous in practical policy research than in "scientific" research, for in the latter case no one cares because nothing will be done, based on its "impractical" findings, but in the former, people suffer the results of the research error when the policy is implemented. Often the insistence in making the research practical suggests the wrong question. Just as often the "armchair" theoretical hypothesis is the wrong question. I believe that the best hypotheses are set in grounded theory [see Glaser and Strauss (1967], rooted in empirical reality and not too far from practical behavior.

Research, *all research* (both practical-policy and "scientific" research), must be theory-oriented or else it is poor research. Here Hoy is absolutely correct! Good research starts with theory and ends with theory. Why? Exactly for the reason that Hoy seems to ignore when he puts down practical-policy research as "unscientific." Theory must be "tested" against reality. Theorists are fond of saying, "Nothing is so practical as good theory;" I profoundly believe that. With good theory we could predict what is presently unpredictable. To paraphrase Harris (1974), the reason we know so little about the life-styles of man is not due to the lawlessness of the life-styles but because we bestow the greatest rewards upon those who never threaten a fact with a theory. This leads me to wonder whether a theory that points no direction for practice (sooner or later) is a theory at all, or merely a collection of facts.

Hoy takes the policy researchers to task in a single sentence, "So-called policy research, in which the task is to find data to support an existing policy, is not simply poor research, it is shoddy practice." I agree but stated it more strongly in my rebuttal to Pittenger-Deasy (1977). I called it *immoral* and akin to colonial imperialism (Lutz, 1978).

I agree that the era of grantsmanship, with its particular turn during the late 60's toward the pragmatic development of mechanistic technology and the pressing for the adoption of such "innovations," has been a sad and sorry condition for research. Wolcott (1977) documents the results of such "practical" expenditures very well, and their actual impractical consequences.

It seems that in educational administration, and in education in general, professors are really not interested in, or at least not significantly engaged in research. This is due in part to lack of prior training, the general focus in education on what passes for the practical and is certainly non-conceptual, and finally (in spite of rumor to the contrary) the abysmal lack of institutional support for research in colleges of education. I suggest that the general view in most colleges (supported by a university administration that used the college of education to generate tuition to support other more "scientific" endeavors in other colleges) is, "It is OK to do research as long as you keep your student credit hour total up."

Additionally, one often finds some person in education attempting to discover if one pragmatic method is more efficient or effective than another. There is no theory development involved at all. What is going on is a simple quality test of a product not unlike what a small boy might do before he opens a lemonade stand. He might try one pitcher of drink with one cup of sugar against another with two cups and inquire which type is better accepted by his potential customers. For the purposes of this discussion, such an "experiment" is not considered research.

Now I come to the point of strong disagreement. Even here there are points of common sentiment that I share with Hoy. Shall we waltz? I strongly disagree, however, with the notion that there is not, nor should there be any practical solution to real world problems as a result of "scientific" research. In passing, I also disagree with the notion that "field studies methodologies," by which I presume Joy means such research as the ethnographies produced by anthropological field methods, should or can be (in the "scientific" sense) hypothesis testing. I do *not* think they should be used to test hypotheses. They should, however, be theory related and are

better suited to the very important task of problem specification and hypotheses generation as opposed to hypotheses testing.

That matter aside, the process of theory generation, of which the minor task of hypothesis testing is but a part, is the most practical thing I can imagine. The falsely stated dichotomy between theory and practice has too long driven the practice of education away from the theory of education and *vice versa*, to the disadvantage of both. I do agree with Hoy that even when research has been done in educational administration, it is most often sporadic, unrelated, and not theory building. It has usually failed to build on the work of others and is almost always non-programmatic.

I know of a few exceptions to this lack of programmatic attempt to build theory, and there are several other lines of inquiry that offer promise of becoming recognized as theory oriented lines of research in educational administration. Hoy mentions one of those that has already established itself as a theory oriented line of research. This is the work in "pupil control ideology." Using it as an example Hoy attempts to demonstrate why the research should not be asked to provide any practical solutions to problems. In my view, Hoy's argument faults the pupil control ideology research more than making a good case for his position.

Dozens of studies have been conducted and their results published in this line of research. As pointed out by Hoy, we know a wealth of Person product-moment correlations between pupil control ideology and behavior, and many other educational variables. But we know virtually nothing about how to make teachers more or less humanistic or whether or not, if we could, how that might affect the educational outcomes of pupils. While I certainly do not contend that research should be judged or funded solely upon its ability to show direct or immediate pragmatic benefits, I do strongly contend that It should be grounded in the empirical realities of education (which pupil control is) and offer some insight into or hope of eventually improving practice (which Hoy points out, pupil control ideology researchers have not attempted to do). If that is their choice, it must also remain their concern. I believe there are several practical suggestions inherent in the pupil ideology literature, unexplored by those engaged in the research. It may be then that such research may finally contribute to the practice of education as others build on their work. But I believe it incorrect and even harmful, to so define "scientific" research so that that research becomes scientific and all other types of research become "unscientific." If we accept this unscientific label we must also accept, so far as universities are concerned (where most research goes on) the classification of second rate, with the attending consequences.

What then is research and what makes it scientific or unscientific? Much of my orientation toward research comes out of the traditions in social anthropology. Geertz (1973) speaks of ethnology as "thick description." By this he means careful and complete description--so complete it permits analysis, explanation and the development of meaning. He says

> The claim of attention of an enthnographic account [might we say its scientific worth] does not rest on its author's ability to capture primative *facts* [my emphasis] in far away places and carry them home like a mask or a carving, but on the degree to which he is able to clarify what goes on in such places, or to reduce puzzlement....(p.16)

Geertz seems to be saying that the worth of research lies not in a collection of mere facts but in the explanation, the practical meaning, such facts can supply. He emphasizes the same notion later:

> ...analysis is (or should be) guessing at meanings, assessing guesses, and drawing explanatory conclusions from the better guesses, not discovering the Continent of Meaning and mapping out its bodyless shape. (p. 20)

Discussing the temptation to substitute facts for patterns and logic for meaning, Geertz speaks of the attempt "to set King Reason back upon his throne in the guise of the Cerebral Savage...." He suggests that

> However much it is set round with symbolic logic, matrix algebra, or structural linguistics, can we--after all that has happened since 1762--still believe in the sovereignty of the intellect?...For anthropologists are forever being tempted...out of the libraries and lecture halls, where it is hard to remember that the mind of man is no dry light, into "the field" where it is impossible to forget it. (pp. 358-359)

Would that the same could be said for the researchers in school administration.

Speaking of the unlikelihood that the simple laboratory relationship will prove useful in the real world he says

> Referring as it does to both formal principles of reasoning and to rational connections among facts and events, "logic" is a treacherous word....The reason for this is, of course, that meaning is not intrinsic in the objects, art, processes, and so on, which bear it, but--as Durkheim, Weber and so many others have emphasized--imposed upon them; and the explanation of its properties must therefore be sought in that which does the imposing--men living in society. The study of thought is, to borrow a phrase from Joseph Levenson, the study of men thinking; and as they think not in some special place of their own, but in the same place--the social world--that they do everything else, the nature of cultural integ-

> ration, cultural change or cultural conflict, is to be probed for there; in the experiences of individuals and groups of individuals as, under the guidance of symbols, they procure, feel, reason, judge, and act. (pp 404-405)

Of course Geertz was referring specifically to anthropology and ethnography. I think there is a message there for educational administration and those who see scientific research as separate from the profane world of practice.

Sahlins speaks about the relationship between the facts of a cold science untainted by the clutter of practical events of a real world and the world of practical meaning. He says

> At first glance the confrontation of the cultural and material logics does seem unequal. The material process is factual and "independent of man's will"; the symbolic, invented and therefore flexible. The one is fixed by nature, the other arbitrary by definition. Thought can only keel before the absolute sovereignty of the physical world [equivalent one supposes to "scientific" research]. But the error consists in this: that there is no material logic apart from the practical interest and the practical interest of men in production is symbolically constituted. The finalities as well as the modalities of production come from the cultural side: the material means of the cultural organization as well as the organization of material means. We have seen nothing in the way of their capacity to satisfy a material (biological) requirement [that] can explain why pants are produced for men and skirts for women, or why dogs are inedible but the hindquarters of the steer are supremely satisfying of the need to eat. (Sahlins, 1976)

Perhaps there is here a "middle-ground" for the debate about research in educational administration (see Griffiths, 1975, and Greenfield, 1976). Without suggesting that either Griffiths or Greenfield is a supporter of my position or of Hoy's, or contending that their debate was about the issues discussed here, it might be posited that neither the phenomenological nor the empirical explanation is incorrect, nor is either sufficient in itself. Neither is the only "scientific" approach nor is either "unscientific" in the broad sense. Elsewhere I have argued (Lutz, 1975), following the arguments proposed by Caws (1974) that it is the explanatory model, based on both the operational and the representational models that is at the heart of theory development. It is the explanatory model, combining the reports of informants (the natives) about the phenomenon as they perceive it and understand it, together with the operational observations of the scientists (the anthropologists), that most completely describes and explains the phenomenon in the culture or organization.

Any hypothetical example may serve to demonstrate the way the representational model operates, with the operational model, in order to develop an explanatory model of reality and solve a problem. Suppose in a certain tribe it is believed that smoke from the ritualistic pipe, blown into the air in the correct manner, under the proper conditions, by the shaman, establishes a proper relationship with the gods--therefore (when there is a proper relationship with the gods) it rains, the crops grow, and the tribe prospers. These conditions are established by scientific research and from the representational model.

Suppose that the region is experiencing a three-year drought, the crops are failing and there is the threat of starvation in the area. A "scientist" goes in and scientifically establishes that the crops that are currently planted cannot grow during the regular and periodic droughts; that irrigation methods are possible and/or other subsistence crops will thrive even during the drought periods. This is the empirical model.

It is clear that the representational model will not solve the crisis. The empirical model will, however, upset the equilibrium of the society and therefore be impossible to force upon the people. The explanatory model provides several possible solutions, however. First we would, at least presently, do nothing to attempt to disuade the society of their notion that the smoke blown by the shaman establishes the proper relationship with the gods. We might try to convince them, however, that while the gods send the rain they have no notion that the lack of rain is causing them a hardship. Rather, the gods have diminished the rainfall because of another unrelated problem. Perhaps the diminished rainfall is due simply to the magic of powerful shaman of another tribe but unrelated to this tribe's problem. If so, and if there is no taboo against "scarring the earth," the tribe might be persuaded to irrigate their crops and thereby further please the gods with the feasts that the plentiful produce would make possible. Or, if digging trenches is impossible because of taboo, the tribe might be persuaded that a very positive relationship exists with the gods and the present condition is simply not correctly understood. Rather than punishing them, the gods are trying to favor them by getting them to introduce other crops (perhaps in addition to their standard ones) and, therefore, sent the drought. Thus, the tribe may be willing to plant some drought resistant crops.

I would contend that the representational model above (correctly researched) was scientific but alone was not helpful in solving the problem. The operational model (correctly researched) was scientific but could not alone provide a usable solution for

solving the problem. The explanatory model (combining both models) was capable of proposing several solutions; it was the result of scientific research and analysis, and afforded possible solutions to a practical problem.

To engage in either separately is but "half a loaf." To label either type of research as "unscientific" is arrogant. To ignore either type is foolhardy! Let me hasten to add that, of course, this little example is superficial and incomplete, but I think it is illustrative of the problem of labeling the non-experimental, deductive approach, or the quest for solutions, as unscientific. The fact is that the statements inductively produced in the representational model permitted one to deductively propose the "if-then" solution to the practical problem. If research cannot *finally* offer some speculation about possible solutions to human problems we may finally end up with nothing left in the world except libraries.

Homans (1960, pp. 10-11) states, "...science... needs a set of general propositions about relationships.... But much modern sociological theory seems quite satisfied with itself when it has set up its conceptual scheme. The theorist shoves different aspects of behavior into his pigeonholes, and cries, 'Ah-ha!,' and stops." Homans might have continued that the zero order correlations describing the relationship between pairs of pigeonholes--while a step beyond the pigeonholes--is still far short of theory. Implicit in Homan's statements about theory is the notion that the theory should explain behavior and offer some prediction about future behavior. This is the theory that is described as "the most practical thing."

Every isolated piece of cancer research cannot hope to show how it will prevent or cure cancer. Some basic research may, in fact, make unanticipated contributions to other medical problems. The most significant break-through may come from some research not directly related to the attempt to find a cure or prevention for cancer. But to openly contend that cancer research is unscientific if it is structures so as to hope that it will make some contribution to the solution of that problem would be foolhardy!

I would agree that there is no reason to be concerned that articles such as this article and Hoy's will suddenly make research and theory king in education, thereby driving the concern for practice out of the door. What I fear Hoy's article might do is to drive the researchers and (if we can claim so grand a thing) the theorists, on the one, hand, and the practitioners on the other, further apart. The result is that practice ignores research and theory, and theory and research ignore practice--to the detriment of each. Thus, we remain in the same position where we have been for at least two decades. Most of the time, researchers and practitioners ignore one another, but occasionally they notice each other long enough to call one another names.

Again, I certainly would not suggest that research be required to demonstrate its practical application in order to proceed. Einstein, it seems, originally had no idea of the practical applications of MC^2. The fact is, there is something practical in simply knowing. But I would not cast out of the priesthood researchers who develop a practical statement about the behavior or lifestyles of mankind based on their research findings.

In our recent book (Lutz and Iannaccone, 1978, p. 7) Iannaccone and I have taken a position regarding research, theory, and practice, that I think is quite different from what Hoy seems to day. Describing some fifteen years of pragmatic research that resulted in what is termed "The Dissatisfaction Theory of Democracy" we state that,

> ...the rational process of research is not separated from practice and practical reality in education... research and practice are inextricably interrelated, each advancing the other, as the right leg advances the left leg, but then must pause to allow the left to advance the right. Only in this way do "we" make smooth progress, together, not separately. The alternative is a separate, segmented and halting hopping that balances precariously at best and usually falls of its own weight.

While agreeing with many of the statements Hoy makes about research, I come to a very different conclusion about the relationship between research and practice. If research fails to suggest possible implications for practice based on its findings, or if it arrogantly ignores practice as a fruitful source of meaningful hypotheses, it does so at its own peril, for then it not only fails to improve practice, but also fails to improve itself.

I agree with the position regarding theory and practice, particularly in schools of education, recently taken by Nathan Glazer (1978), "The disciplines, after all, in some respect must model the real world and must be compared to it." To do so does not make research less scientific but more so. □

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Theory

Research

Practice